Why Are Consumers Inattentive to Hidden Costs?

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Abstract

Using a series of experiments eliciting willingness to pay for a common consumer product (a reusable razor) that has hidden costs (replacement blade cartridges), we test whether inattention to hidden costs arises because consumers lack knowledge of, forget about, or have incorrect information about hidden costs. Much of the existing work documenting inattention has difficulty differentiating between these hypotheses. We find that none of these explanations can account for inattention: participants demonstrate familiarity with the hidden costs of buying a razor (ruling out lack of knowledge), yet willingness to pay is insensitive to reminders (indicating that consumers are not forgetful) and to the provision of objective information about costs (ruling out incorrect information). We find suggestive evidence that consumers do not aggregate the value of a product’s individual attributes or components in a way that is consistent with their stated valuation of the whole product. This finding suggests that aggregation errors may contribute to the inattention that we and other researchers have documented.

1 Introduction

Even decisions as simple as what to buy at the drug store involve hidden costs, or costs that are not explicitly written on a product’s sticker price (at least in the U.S. where sales taxes are only applied at the register).\(^1\) Sales tax is just one of many examples of hidden costs

\(^1\)By hidden costs, we mean costs associated with buying or using the good or service purchased that are above and beyond the sticker price of the good or service. Some examples include sales tax, add-ons such as printer ink cartridges, and tips for restaurant service. We use the word “hidden,” but others have used “not salient,” “shrouded,” or “additional.”
that range from use costs (like energy consumption) to add-ons (like printer cartridges) to additional charges (like ticket processing fees and shipping costs). Prior research has documented behavior consistent with inattention to hidden costs in these and other contexts (e.g. [Hossain and Morgan 2006] [Chetty et al. 2009] [Finkelstein 2009] [Allcott and Taubinsky 2015] [Bradley and Feldman 2018] [Taubinsky and Rees-Jones 2018]). In this paper we ask why consumers ignore hidden costs by beginning a systematic investigation of consumer choice in contexts that involve hidden costs.

Prior work offers some hypotheses for this inattention. For example, Chetty et al. (2009) conclude that “[t]here are two potential explanations for the finding that consumers underreact substantially to taxes that are not included in posted prices. One is that customers are uninformed about sales tax rates. Showing the tax-inclusive price tags may have provided new information about tax rates. An alternative explanation is that salience matters: individuals know about taxes when their attention is drawn to the subject, but do not pay attention to taxes that are not transparent while deciding what to buy.” A number of papers have investigated the hypothesis that consumers are uninformed by comparing choices made by a treatment group, which is given objective information about hidden costs, to a control group, which is not (for two recent papers that find no effect of information provision see Beshears et al. 2011 and Allcott and Knittel 2017). There are other potential explanations as well. A suggestion inspired by the work summarized in Mullainathan and Shafir (2013) is that the consumer ignores hidden costs not because she is uninformed or inattentive, but because calculating the full cost of each item takes too much cognitive bandwidth. Tax inclusive price tags, for example, eliminate the need to spend cognitive resources calculating total cost, so the consumer does not underreact to the hidden cost.

The existing work has difficulty differentiating between these and other possible explanations. Again using sales taxes as an example, adding tax inclusive price tags reminds and informs consumers about the sales tax, draws the consumer’s attention to the sales tax, and eliminates the cognitive effort needed to calculate tax inclusive prices all at once. Re-
search using experiments that provide information on hidden costs to participants randomly assigned to the treatment group has a similar difficulty, as the information both provides a reminder and gives information. Our contribution is to provide a direct test of several explanations. We ask whether consumers appear inattentive because they a) do not know about hidden costs (knowledge), b) know, but forget, about hidden costs (memory), or c) know and recall, but have incorrect information about, hidden costs (information). Clear tests of these explanations provide the basis necessary for conducting further investigations into sources of inattention.

To answer our question, we conduct a series of experiments designed to allow us to differentiate between the three hypotheses noted above. In all of our studies, participants recruited on MTurk are asked to indicate their willingness to pay for a reusable razor, a common consumer product with notable hidden costs in the form of replacement cartridges, using an incentive compatible mechanism. We experimentally vary the information participants are shown prior to making their decisions, either providing a simple reminder, a reminder that contains objective information about hidden costs, or neither a reminder nor objective information. We also elicit participants’ perceptions about hidden costs.

We find that willingness to pay is insensitive to reminders and to the provision of objective information about hidden costs. These results suggest that neither lack of knowledge, imperfect memory, nor incorrect information can explain inattention to hidden costs; however, these results are also consistent with the interpretation that participants in all three experimental groups pay equal amounts of attention (rather than equal amounts of inattention) to hidden costs. To investigate this alternative interpretation, we conduct two subsequent studies in which we manipulate the objective amount of the hidden cost itself, rather than altering the attention drawn to the cost. The results of these studies support the hypoth-

\(^2\)Another approach to understanding why consumers may be inattentive is to examine heterogeneity in attention by a consumer’s observable characteristics. The idea in this approach is that by understanding the correlates of attention, one can begin to form and then test hypotheses about the causes of attention. We view this approach as complementary to the method we use (see e.g. Benjamin et al. 2013, Stango and Zinman 2014, Abeler and Jäger 2015).
esis that the findings from our first study are the result of equal inattention and not equal attention.

Research on the sources of inattention should matter to every company, organization, government, and person. By ignoring hidden costs, consumers may be inadvertently making optimization mistakes. A better understanding of why consumers neglect hidden costs will highlight the conditions under which consumers are most likely to make these mistakes. Thus, identifying these conditions is valuable to any entity trying to influence consumers (for better or worse) and could inform regulation on how to protect consumers from those looking to exploit their behavioral biases. For example, if consumers ignore hidden costs because they have incorrect information, policies that focus on correcting misinformation would reduce optimization mistakes. If instead consumers ignore hidden costs because the calculation is cognitively costly, providing the calculation for the consumer would reduce optimization mistakes. Moreover, understanding why consumers are inattentive helps accurately predict effects on behavior and consumer welfare. An example that has been in the news recently is the idea of a soda tax. Would students buying food at the dining hall or cafeteria buy fewer bottles of soda if the price tag said $2.68 instead of $2.49, or are signs reminding students of the tax rate sufficient to affect demand? If consumers are forgetful or misinformed, soda taxes can successfully reduce demand (and increase welfare by reducing optimization errors) with an information campaign; however, if instead consumers ignore hidden costs because the calculation is cognitively costly or because attention requires a visual reminder, politicians may overestimate the effect on demand (and on welfare) of an information campaign. The value of our research is ultimately about determining how mutable attention is. Mutability informs how well behavioral interventions will work and any effects in general equilibrium, as limited attention could help or hurt market efficiency depending on the context.

The rest of the paper is organized as follows: the subsequent section describes our main methodology, followed by a discussion of our results. We next detail the design and out-

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3 See Handel and Schwartzstein (2018) for a discussion of the importance of understanding the sources of inattention.
come for two additional studies that supplement the findings in our main study. Section 5 concludes.

2 Methods

We use experiments to answer our research question by carefully manipulating the content participants see prior to making the main decision of interest. We designed and ran all experiments as online surveys and recruited participants using Amazon Mechanical Turk (MTurk), which provides a matching service between entities (including researchers) seeking individuals to complete tasks (such as research studies) and individuals willing to complete these tasks. We chose to use MTurk not only for convenience, but also because we are interested in a sample that is not composed solely of university students. All participants were compensated for their participation.

The purpose of the main study, described in this section, is to test whether consumers ignore hidden costs associated with purchase decisions because they a) do not know that there are hidden costs, b) do not remember that there are such costs, or c) do not know the true magnitude of these costs. In this study, participants were asked to make choices about whether or not to buy a consumer product at various prices. We experimentally varied the information each participant was shown prior to asking the participant to make his or her decisions.

Before detailing the survey methodology, there are a few design features to highlight: First, the product we ask participants to evaluate is a reusable razor. We chose this product because this is a common consumer product that has significant hidden costs in the form of replacement blade cartridges. Second, we use an incentive compatible random order multiple price list to elicit each participant’s valuation of, or willingness to pay for, the product.

4 We give participants the following explanation of incentive compatibility: “First, you will answer questions about your willingness to pay for the razor at various price levels. If you are selected to possibly buy the razor you will earn a cash bonus, and we will then generate a random price for the razor using a computer. If you indicated that you were willing to pay at least the randomly-generated price, we will send you the
method only allows us to observe a range for each participant’s willingness to pay; however, based on conversations with researchers who have used both multiple price lists and methods directly asking for willingness to pay, we opted for the multiple price list because the choices in a multiple price list are more reminiscent of choices consumers face in an actual store than are those in a direct valuation question. In order to convert the ranges we observe to numbers we can use in analysis, we define each participant’s maximum willingness to pay to be the largest value of the multiple price list at which the participant indicates that he or she is willing to buy the product. For example, if a participant indicates that she is willing to buy the good at $9, $6, and $11, but not at $14 or $12, we assign her maximum willingness to pay to be $11. Note that this will be a lower bound on her true willingness to pay. If a participant indicates that she is never willing to buy the product for the prices we offer, we randomly draw a number from 0 to the lowest price on our price list from a uniform distribution and call this maximum willingness to pay.

2.1 Design

Based on some pilot testing and the resulting power calculations, we ran the following study with N = 1,028 participants recruited online in late February 2018.\footnote{We targeted around 1,000 participants because we wanted to ensure we would have at least 80\% power to detect roughly 50 cent differences in willingness to pay at a 95\% confidence level given a design with three conditions. We used data from a small pilot to determine a reasonable difference in willingness to pay and also a reasonable standard deviation to use in our power calculations. We initially recruited 1,085 participants, but 57 of these did not pass the initial screening for interest in the product used in the study.}

1. Participant screening: Participants had to indicate that they were frequently in the market for the consumer product we use in the study. Because reusable razors are typically marketed by gender, we allow participants to indicate interest in either a razor in the mail and deduct the randomly generated price from your cash bonus. Note that you will not pay the amount you indicate being your maximum willingness to pay - you will only ever pay the randomly generated price. So it is in your best interest to be honest about which prices are appealing to you for the razor and which are not. For example, if you say you are only willing to pay a maximum of $1 for the razor when you are truly willing to pay up to $10 for the razor, then you will regret it if the randomly selected price is, say, $6 - in such a case you would not get the $6 razor even though you truly do want it at that price (since $6 < $10).
men’s or a women’s razor, and then show them the product that corresponds to their selection throughout the study.

2. Consent: Participants gave consent to participate in the research study to comply with the Institutional Review Board.

3. Instructions: Participants are told that they will see a consumer product, its description, and a list of prices. The participant is told that he or she must indicate whether he or she is willing to buy the product at each listed price and we explain the mechanism that makes being truthful incentive compatible (see footnote 4). We also ask participants to refrain from looking up the product during the study. Finally, to avoid issues related to differences in local sales tax rates, we tell participants to assume that there is no sales tax on the products they see in this study.

4. Experimental variation in information: We experimentally manipulate the information each participant is given prior to making his or her purchase decisions. We have three conditions:

   (a) Control: Participants are given no additional information.

   (b) Reminder: Participants are reminded that one must buy replacement razor cartridges when the blades get dull.

   (c) Reminder and information: Participants are reminded that one must buy replacement razor cartridges when the blades get dull. Participants are also given objective information about the cost of cartridge replacements (around $16 for both men’s and women’s products) and the typical frequency of replacement (about four cartridges every 4 - 5 months). We made an effort to ensure that this information was accurate.

5. Attention check: Participants are given a short comprehension quiz to ensure that they have read the instructions.
6. Purchase decisions: Participants indicate whether they are willing to buy the razor at each price in a list of prices. This gives a range of willingness to pay. The prices included in the multiple price list are 65, 75, 90, 95, 100, 105, 110, 125, and 135 percent of the product’s market price at the time of our study, which was $11.19. To limit the chance that a participant inadvertently gives inconsistent responses, we warn participants if their responses are inconsistent. For example, stating that one is willing to buy the product for $10 but not for $6 is an inconsistent response. Once we provide this warning, we do allow participants to continue even if their responses remain inconsistent.

7. Cost and frequency perceptions: Participants give their best guess of the cost of a pack of four replacement cartridges and how often they personally would need to buy these replacements. (For participants in the reminder and information condition, this question is more of a recall test since these participants were given the objective cost and replacement frequency information at the beginning of the study.) All participants at this point were shown (or reminded of) the objective information about cartridge cost and average replacement frequency, and were asked to indicate their level of surprise about this information on a scale from 1 (not surprised) to 7 (very surprised). We also asked participants what their guess for how often the average person would need to buy a pack of four replacement cartridges would have been without the objective information we provided. In addition, participants in the reminder and information condition then also indicated what they would have guessed for replacement cartridge cost if they had not been shown the objective information.

8. Survey end: Participants are thanked for their participation.

If consumers are inattentive to hidden costs because they are ignorant of the existence of these costs, we based this price on the product’s price on Amazon. Because we allowed consumers to choose a men’s or women’s razor, to simplify the survey code we chose a base price between the actual prices of a men’s and women’s razor.
of these costs, providing information about these costs should reduce willingness to pay for the product. If consumers are inattentive because they are forgetful, then providing a simple reminder that the product has hidden costs should reduce willingness to pay. If instead consumers are inattentive because they have incorrect information, then the willingness to pay between the reminder and the reminder and information conditions should differ. Because we exogenously manipulate the information each participant sees, we can identify the effects of providing different types of information by comparing sample means.

3 Results

The key question we want to answer is whether providing a reminder and/or information about hidden costs affects willingness to pay. If inattention is not the result of ignorance about these costs, forgetting these costs exist, or having inaccurate information about the magnitude of these costs, then willingness to pay will not differ by condition.

This is the result that we find. Table 1 summarizes our main findings, showing the mean and standard deviations of willingness to pay for each condition. The sample in this table, and all analyses in the main text, excludes participants who did not pass the attention check and also excludes by necessity participants who gave inconsistent responses on the multiple price list. As a result, our analysis sample consists of $N = 865$ participants.

<table>
<thead>
<tr>
<th></th>
<th>Control</th>
<th>Reminder</th>
<th>Reminder and info</th>
</tr>
</thead>
<tbody>
<tr>
<td>max WTP</td>
<td>9.821</td>
<td>9.848</td>
<td>10.09</td>
</tr>
<tr>
<td></td>
<td>(0.137)</td>
<td>(0.155)</td>
<td>(0.166)</td>
</tr>
<tr>
<td>$N$</td>
<td>304</td>
<td>284</td>
<td>277</td>
</tr>
</tbody>
</table>

Note: Sample excludes participants who did not pass the attention check and who gave inconsistent responses on the multiple price list. Standard errors in parentheses.

Looking at the means suggests that willingness to pay does not differ by condition. This
is confirmed in Table 2 which shows our results in regression form and also includes a control for the razor type (men’s or women’s) each participant selected. Relative to the control condition (the omitted category), there is no additional effect on willingness to pay of providing either a reminder or a reminder with objective information. Moreover, pairwise comparisons of means across conditions adjusted for multiple testing yield the same result, indicating that there are no statistically significant differences in mean willingness to pay between any pair of conditions. Thus, our results suggest that neither knowledge, memory, nor information can account for inattention to hidden costs.

Table 2: Main study: willingness to pay by condition

<table>
<thead>
<tr>
<th></th>
<th>max WTP</th>
</tr>
</thead>
<tbody>
<tr>
<td>Reminder</td>
<td>-0.0175</td>
</tr>
<tr>
<td></td>
<td>(0.212)</td>
</tr>
<tr>
<td>Reminder and information</td>
<td>0.242</td>
</tr>
<tr>
<td></td>
<td>(0.213)</td>
</tr>
<tr>
<td>Men’s razor</td>
<td>0.727***</td>
</tr>
<tr>
<td></td>
<td>(0.175)</td>
</tr>
<tr>
<td>Constant</td>
<td>9.484***</td>
</tr>
<tr>
<td></td>
<td>(0.168)</td>
</tr>
<tr>
<td>R-sq</td>
<td>0.0218</td>
</tr>
<tr>
<td>N</td>
<td>865</td>
</tr>
</tbody>
</table>

Note: * p < 0.1, ** p < 0.05, *** p < 0.01. Standard errors in parentheses. The omitted condition is the control condition. Sample excludes participants who did not pass the attention check and who gave inconsistent responses on the multiple price list.

In addition to collecting information about willingness to pay, we also collected various estimates about perceived hidden costs by asking participants to estimate the cost of a pack of four replacement cartridges and how often the average person, and they personally, would have to buy these replacements. The means and standard deviations are shown in
Table 3. The first two rows describe participants’ estimates of how often (in months) the average person would need to buy a pack of four replacement cartridges. In all conditions, participants were asked this question after having been shown objective information about average replacement frequencies and replacement cartridge costs, so these are individuals’ ex-post predictions of their own ex-ante responses. The results indicate that participants are roughly accurate in their perceptions, as they indicate that the average person needs to buy four new replacement cartridges every 3 - 4 months (compared to every 4 - 5 months, which is our best estimate from reading company websites and reports). Further, pairwise comparisons of means across conditions adjusted for multiple testing indicate no difference between the three conditions. The third and fourth rows show participants’ estimates of his or her own replacement frequency. In this case, only the reminder and information group had seen objective cost and replacement frequency information before answering this question. Again, there are no statistically significant differences between means in any condition, which suggests that lack of accurate information is not driving inattention. Note that, both a t-test of the estimated replacement frequency for the average person and the estimated replacement frequency for “you personally” pooled across all conditions and pairwise comparisons of the difference between the “average” and “personal” estimates (adjusting for multiple testing) reveal that people systematically think that they need to buy replacements less often than the average person. This might reflect some kind of self-serving bias. Still, because some of these cost and frequency estimates are counterfactuals (i.e. we ask participants what they would have guessed before seeing the objective information), we do not want to overinterpret these results.

The last four rows show information about estimates of the cost of a pack of four replacement cartridges. The key result from these rows is that people generally underestimate the true cost of replacements (roughly $16 for a pack of four), and thus the hidden costs. Several other notable patterns emerge: the fifth row shows means of cost estimates made before being shown objective cost information for the control and reminder conditions. Par-
<table>
<thead>
<tr>
<th></th>
<th>Control</th>
<th>Reminder</th>
<th>Reminder and info</th>
</tr>
</thead>
<tbody>
<tr>
<td>Freq. (avg person; months)</td>
<td>3.584</td>
<td>3.593</td>
<td>3.971</td>
</tr>
<tr>
<td></td>
<td>(0.133)</td>
<td>(0.161)</td>
<td>(0.168)</td>
</tr>
<tr>
<td>Freq. (personally; months)</td>
<td>4.266</td>
<td>4.187</td>
<td>4.469</td>
</tr>
<tr>
<td></td>
<td>(0.213)</td>
<td>(0.223)</td>
<td>(0.213)</td>
</tr>
<tr>
<td>Cost (w/o information; $)</td>
<td>11.08</td>
<td>10.25</td>
<td>12.61</td>
</tr>
<tr>
<td></td>
<td>(0.295)</td>
<td>(0.287)</td>
<td>(0.267)</td>
</tr>
<tr>
<td>Cost (w/ information; $)</td>
<td></td>
<td></td>
<td>12.95</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>(0.267)</td>
</tr>
<tr>
<td>N</td>
<td>304</td>
<td>284</td>
<td>277</td>
</tr>
</tbody>
</table>

Note: Sample excludes participants who did not pass the attention check and who gave inconsistent responses on the multiple price list. Standard errors in parentheses.

Participants in the reminder and information condition had seen the objective cost information, but were asked to state what their estimate would have been if they had not been shown the information. Pairwise comparisons of means across condition (adjusting for multiple testing) indicate that there is no difference between the control and reminder conditions, but that the mean for the reminder and information condition is significantly larger than the means in the other two conditions. This again is consistent with a self-serving bias, as participants in the reminder and information condition gave estimates closer to the objective information (which they knew when making the estimate), despite being asked to give their estimate as if they had not already seen the information. The last two rows corroborate this interpretation. Participants in the reminder and information condition were also asked to essentially recall the cost information they were shown. This estimate is not statistically significantly different from the “as if” estimate. The estimate is also larger than cost estimate means for the control and reminder conditions, suggesting hindsight bias: participants erroneously think they understand costs correctly ex-ante, whereas in reality they are being influenced by the information we provide.
The results thus far are consistent both with equal *inattention* and equal *attention* being paid to hidden costs across conditions. We believe that we can rule out this explanation: the average estimated replacement cartridge cost for a pack of four cartridges in the control and reminder conditions are $11.08 and $10.25 respectively (which are not statistically significantly different). The averages in the reminder and information condition are $12.61 (the “as if” estimate) and $12.95 (the recall estimate), which are not significantly different. The difference in estimates between the control and reminder conditions and the reminder and information condition *is* statistically significant. This significantly higher cost perception in the reminder and information condition indicates that the information we provided did affect perceptions of costs, but did not affect willingness to pay (see Tables 1 and 2), which suggests equal inattention, rather than equal attention, to hidden costs. Still, in the next section we describe two studies that provide additional measures of attention that allow us to more directly test for equal attention or inattention.

# 4 Equal attention or equal inattention

To test whether the prior results are driven by equal attention or equal inattention, we conduct two studies manipulating the hidden cost associated with the consumer product. By manipulating the hidden cost itself, rather than just information about the cost, equal attention predicts that products with different hidden costs will have different valuations, whereas equal inattention predicts that such products will have equal valuations.

## 4.1 Changing the bundle

In the first of our two robustness checks, we ask participants to make two choices: one about a razor stick bundled with one replacement cartridge and a second decision about a razor stick bundled with two replacement cartridges. The two-replacement bundle objectively has a lower hidden cost than the one-replacement bundle or similarly an objectively higher value
(because the consumer has to buy one fewer replacement cartridge when purchasing a two-replacement bundle). This study also helps address a limitation of the main study, which is that when a consumer is shopping for a particular product, the consumer rarely evaluates a product in isolation. That is, in a more natural setting, the consumer shopping for a product asks herself which of the many brands, versions, and bundles she would like to buy. Because we ask each participant in this study to make two choices about different bundles, this design more closely resembles a real-world choice, though we readily acknowledge that the mapping is not perfect.

4.1.1 Design

As in the main experiment, we recruited participants on MTurk. The design features are similar to those in the main experiment. We ran the following study on N = 402 participants in June of 2018:

1. Participant screening: as in the main study.

2. Consent: as in the main study.

3. Instructions: as in the main study.

4. Attention check: as in the main study.

5. Purchase decision: as in the main study, with a few alterations. First, we expanded the prices in the price list to 60, 65, 75, 90, 95, 100, 105, 110, 125, 135, and 140 percent of the product’s current price at the time of our study, which for this study was $10.12 (see again footnote 6). Second, participants made purchase decisions about two products: a razor stick bundled with one replacement cartridge, and a razor stick bundled with two replacement cartridges. Half of the participants were randomly selected to first make

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7We initially recruited 425 participants, but 23 did not pass the initial screening. We chose this sample size based on power calculations incorporating information from prior studies on standard deviation to comfortably get at least 80% power to detect differences in willingness to pay of about $1, which itself is about a quarter of the actual cost of one replacement cartridge.
a decision about the one-replacement bundle (condition: choice order one then two), and the other half first made a decision about the two-replacement bundle (condition: choice order two then one).

6. Cost and frequency perceptions: Participants were asked to indicate their best guess of the cost of a pack of four replacement cartridges and how often they personally would need to buy a pack of four replacements.

7. Survey end: as in the main study.

If consumers were paying attention to hidden costs, the difference in willingness to pay between the one- and two-replacement bundles should be equal to a participant’s perception of the cost of one cartridge (inferred from his or her stated estimate of the cost of a pack of four replacement cartridges).

4.1.2 Results

As before, our results exclude participants who gave inconsistent answers in the multiple price list and who did not pass the attention check, resulting in N = 281 participants, each of whom made two purchase decisions (so we have two observations per person).

Table 4: Changing the bundle: hidden cost estimates by condition, cartridge replacement frequency and cartridge cost

<table>
<thead>
<tr>
<th>Condition: choice order 1 then 2</th>
<th>Condition: choice order 2 then 1</th>
</tr>
</thead>
<tbody>
<tr>
<td>Cost ($)</td>
<td>11.22 (0.459)</td>
</tr>
<tr>
<td>Freq. (personally; months)</td>
<td>4.651 (0.357)</td>
</tr>
<tr>
<td>Observations</td>
<td>139</td>
</tr>
</tbody>
</table>

Note: Sample excludes participants who did not pass the attention check and who gave inconsistent responses on the multiple price list. Standard errors in parentheses.

We find that generally participants underestimate the true cost of replacement cartridges (about $11 for a pack of 4 cartridges rather than the true cost which is about $16, see Table
Note that these estimates do not differ by condition (i.e. order of purchase decisions), and that they are consistent with the estimates from the first study (see Table 3). These estimates imply that participants infer the cost of one replacement cartridge to be about $2.75 ($≈ \frac{11.00}{4}$).8

Figure 1: Changing the bundle: willingness to pay by product and condition

Note: Sample excludes participants who did not pass the attention check and who gave inconsistent responses on the multiple price list. 95% confidence intervals shown.

More importantly, using both within and between participant comparisons, we find that the difference in willingness to pay between the two- and one-replacement bundles is statistically significantly less than the inferred cost of one cartridge. These results are summarized in Figure 1. Using only the first decision participants made (left most and right most bars in Figure 1), while participants are willing to pay more for the two-replacement bundle, the difference in willingness to pay is not statistically significant, and the confidence interval

8One possibility is that willingness to pay for one cartridge differs from \frac{\text{willingness to pay for a four-pack}}{4}; however, assuming diminishing marginal utility, if anything the willingness to pay for only one replacement cartridge would exceed \frac{\text{willingness to pay for a four-pack}}{4}, making $2.75 an underestimate of participants’ true valuation of one replacement cartridge.
of the difference does not include $2.75, the inferred cost of one cartridge (95% confidence interval of the difference in means = [−0.10, 1.07]). The two within-condition comparisons tell a similar story (compare the two light grey bars and the two dark grey bars): while in both cases (using paired t-tests) the difference between willingness to pay for the one- and two-replacement bundle is statistically significant, the 95% confidence interval again does not contain the inferred cost of one cartridge ([0.42, 0.88] for the one-then-two ordering and [0.17, 0.61] for the two-then-one ordering). Finally, because willingness to pay for the one(two)-replacement bundle does not differ by condition (compare the two left most or two right most bars in Figure 1), we can pool the two conditions. This pooled estimate of the difference in willingness to pay indicates that people are willing to pay more for a two-replacement bundle, but not by an amount that is even close to the reported inferred cost of one cartridge (95% confidence interval of the difference in means = [0.36, 0.68]). These results are confirmed by the regression in Table 5 which controls for whether the participant indicated interest in a men’s or women’s razor. This table shows that: 1) the order of the decisions does not matter (the coefficient on the condition variable is not significant), and 2) participants are willing to pay more for a two-replacement bundle, but by an amount that is statistically significantly smaller than $2.75 (the cost of one replacement cartridge inferred from participants’ responses). These results suggest that participants are not paying full attention to hidden costs and that the results in the first study are driven by equal inattention rather than equal attention.

4.2 Separating the bundle

One potential issue with the prior robustness check is that the manipulation is rather subtle (though the statistically significant differences in willingness to pay for the one- and two-replacement bundles indicate that participants did notice the difference). As an additional robustness check, we make a more blatant manipulation of the products being offered and ask

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9For the bundle with one replacement, the difference in means is $0.09 (se = 0.30), p = 0.76, and for the bundle with two replacements the difference in means is $0.17 (se = 0.29), p = 0.56.
Table 5: Changing the bundle: willingness to pay by product and condition

<table>
<thead>
<tr>
<th>Condition: choice order 2 then 1</th>
<th>max WTP</th>
<th>(0.207)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Condition: choice order 2 then 1</td>
<td>-0.0679</td>
<td></td>
</tr>
<tr>
<td>Product: bundle w/ 2 replacements</td>
<td>0.521*</td>
<td>(0.207)</td>
</tr>
<tr>
<td>Men’s razor</td>
<td>0.553**</td>
<td>(0.207)</td>
</tr>
<tr>
<td>Constant</td>
<td>8.544***</td>
<td>(0.212)</td>
</tr>
</tbody>
</table>

R-sq 0.0235
N 562

Note: * p < 0.1, ** p < 0.05, *** p < 0.01. Standard errors in parentheses. The omitted condition is choice order 1 then 2. Sample excludes participants who did not pass the attention check and who gave inconsistent responses on the multiple price list.

participants to make purchase decisions about each component of the bundle separately (the razor stick and a pack of four replacement cartridges) in addition to the combined bundle (a razor stick with four replacement cartridges). In doing so, we can infer the valuation of each component and compare this to the valuation of the combined bundle.

4.2.1 Design

We recruited participants on MTurk. The design is similar to that in the main experiment. We ran the following study on N = 304 participants in June of 2018[10]

1. Participant screening: as in the main study.

2. Consent: as in the main study.

[10]We initially recruited 324 participants, but 20 did not pass the initial screening. We chose this sample size based on power calculations incorporating information from prior studies on standard deviation to comfortably get at least 80% power to detect differences in willingness to pay of about $2, which itself is much lower than our estimates of what the true valuation differences should be based on estimates from our prior studies.
3. Cost and frequency perceptions: One group of participants was shown the razor and asked only to indicate their best guess of the cost of a pack of four replacement cartridges and how often they personally would need to buy a pack of four replacement cartridges for the razor shown. The study then ended for this group.

4. Instructions: as in the main study.

5. Attention check: as in the main study.

6. Purchase decision: as in the main study, with a few alterations: First, we altered the prices in the price list to 35, 48, 62, 75, 89, 102, 116, 129, 143, 156, 169, 183, 196, 210, 223, and 237 percent of the base price of $10.15 (see again footnote 3). These are roughly evenly spaced percents. We expanded the list to account for the wider range of true market values for the products participants were asked to value because we wanted the options in the price list to be the same in all conditions. Second, participants made a decision about only one of the following randomly assigned products:

   (a) Stick only: Participants were asked to make purchase decisions about only the razor stick with no included cartridges. (We realize that this is not a decision consumers face in reality.)

   (b) Cartridges only: Participants were asked to make purchase decisions about a pack of four replacement cartridges. We asked participants who do not own a razor stick compatible with the replacement cartridges shown to imagine that they owned a compatible stick.

   (c) Stick with 4 cartridges (bundle): Participants were asked to make purchase decisions about a stick that comes with four replacement cartridges.

7. Survey end: as in the main study.

If consumers were paying attention to hidden costs, willingness to pay for the stick only ($wtp_{stickOnly}$) + willingness to pay for the pack of four replacement cartridges ($wtp_{cartridgesOnly}$)
should equal the willingness to pay for the stick that comes with four replacement cartridges ($wtp_{stick+4cartridges}$).

4.2.2 Results

After excluding participants who gave inconsistent answers in the multiple price list and those who did not pass the attention check, we have $N = 164$ participants split among the three conditions with purchase decisions and $N = 75$ participants who were asked to only make replacement cost and frequency estimates.\footnote{We drop the four participants who gave cost estimates above $50.} Table 6 shows that these participants gave (under)estimates of the cost of a pack of four replacement cartridges, and estimates of the frequency of replacement that are similar to these estimates from the prior two studies (see Tables 3 and 4).

<table>
<thead>
<tr>
<th>Estimates</th>
</tr>
</thead>
<tbody>
<tr>
<td>Cost ($$)</td>
</tr>
<tr>
<td>12.44</td>
</tr>
<tr>
<td>(0.910)</td>
</tr>
<tr>
<td>Freq. (personally; months)</td>
</tr>
<tr>
<td>3.873</td>
</tr>
<tr>
<td>(0.350)</td>
</tr>
<tr>
<td>Observations</td>
</tr>
<tr>
<td>75</td>
</tr>
</tbody>
</table>

Note: Sample excludes the four participants who gave cost estimates above $50. Standard errors in parentheses.

Comparing these estimates to the willingness to pay in the other three conditions in this study supports interpreting the results in our main study as equal inattention across conditions. The average and standard error of willingness to pay for each condition is in Table 7. There are two ways to see how the summary measures in this table suggest inattention. If people were paying attention, $wtp_{stickOnly} + wtp_{cartridgesOnly} = wtp_{stick+4cartridges}$, or $wtp_{cartridgesOnly} = wtp_{stick+4cartridges} - wtp_{stickOnly}$. So, we can compare the confidence
interval on a test of the difference $wtp_{stick+4\text{cartridges}} - wtp_{stick\text{Only}}$ to either 1) the mean $wtp_{\text{cartridgesOnly}}$, or 2) the estimate of the cost of a pack of four replacement cartridges from the estimates only group (see Table 6). The 95% confidence interval of the difference in means is from $0.77$ to $3.66$. In both cases, this confidence interval does not include either estimate of the cost of a pack of four replacement cartridges, suggesting that people are not fully attentive when making purchase decisions.

Table 7: Separating the bundle: willingness to pay by condition

<table>
<thead>
<tr>
<th>Condition</th>
<th>stick only</th>
<th>cartridges only</th>
<th>stick + 4 cartridges</th>
</tr>
</thead>
<tbody>
<tr>
<td>max WTP</td>
<td>7.664</td>
<td>11.65</td>
<td>9.879</td>
</tr>
<tr>
<td>(standard error)</td>
<td>(0.569)</td>
<td>(0.649)</td>
<td>(0.451)</td>
</tr>
<tr>
<td>N</td>
<td>53</td>
<td>51</td>
<td>60</td>
</tr>
</tbody>
</table>

Note: Sample excludes participants who did not pass the attention check and who gave inconsistent responses on the multiple price list. Standard errors in parentheses.

As a formal test (and to control for gendered product choices), we estimate

$$wtp_i = \beta_0 + \beta_1 1(\text{condition} = \text{cartridges only}_i) + \beta_2 1(\text{condition} = \text{stick + 4 cartridges}_i) + \beta_3 1(\text{men’s product}_i) + \epsilon_i,$$

where $1(\text{condition} = \text{stick only}_i)$ is the omitted category. We then test whether $\beta_0 + \beta_1 = \beta_2$, and we starkly reject this null hypothesis. As in Table 7, $wtp_{\text{cartridgesOnly}}$ alone exceeds $wtp_{\text{stick+4cartridges}}$, which at first seems odd. We interpret this finding as an indication that consumers may have trouble aggregating the value of individual attributes in a way that is internally consistent. This interpretation reflects the idea that individuals generally do not have an internally consistent model of valuation and that they only decide how to answer a question when they are asked the question directly without thinking about how they would answer related questions. This behavior can thus lead to internally inconsistent responses (see e.g. Chater 2018). Still, our result suggests again that participants are not paying full attention to hidden costs and that the results in study 1 are driven by equal inattention.
rather than equal attention.

Table 8: Separating the bundle: willingness to pay by condition

<table>
<thead>
<tr>
<th></th>
<th>max WTP</th>
</tr>
</thead>
<tbody>
<tr>
<td>cartridges only</td>
<td>4.016*** (0.835)</td>
</tr>
<tr>
<td>stick + 4 cartridges</td>
<td>2.251** (0.705)</td>
</tr>
<tr>
<td>Men’s razor</td>
<td>1.763** (0.620)</td>
</tr>
<tr>
<td>Constant</td>
<td>6.599*** (0.582)</td>
</tr>
<tr>
<td>R-sq</td>
<td>0.174</td>
</tr>
<tr>
<td>N</td>
<td>164</td>
</tr>
</tbody>
</table>

Note: * p < 0.1, ** p < 0.05, *** p < 0.01. Standard errors in parentheses. The omitted category is the stick only condition. Sample excludes participants who did not pass the attention check and who gave inconsistent responses on the multiple price list.

This study is not without limitations. We acknowledge that asking people to state willingness to pay for a stick only is unrealistic, and also that asking people who may not have a compatible stick at home for their willingness to pay for a pack of replacement cartridges is also not realistic. A possible solution is to find participants who do have compatible sticks or to send a stick (or stick with one cartridge) in advance and then to ask about willingness to pay for a pack of four replacement cartridges. Despite these limitations, we believe the evidence in these three studies together makes a strong case for inattention that is not the result of lack of knowledge, information, or memory.
5 Conclusion

Motivated by the question of why consumers are inattentive to hidden costs, we systematically test several proposed explanations for the inattention observed in the literature. Through three experiments, we find consistent and convincing evidence that inattention to hidden costs is unlikely to be the result of lack of awareness, forgetfulness, or inaccurate information. Moreover, individuals appear to be inattentive even to their own stated estimates of a product’s hidden or add-on costs.

Our results have implications both for policy and for accepted methods of inferring values of attributes. To address policy implications, reconsider the example of soda taxes. Since we find that neither reminding individuals of hidden costs nor providing accurate information about these costs affects willingness to pay, merely informing citizens about soda taxes may be an ineffective way to change demand for sugary drinks (since changing behavior is one aim of soda taxes). Directly changing the price consumers see on the price tag may be a more effective method to reduce demand. More generally, our findings indicate that informational campaigns, even if they provide accurate, clear, targeted information about hidden costs, may have a muted effect on behavior.

Turning to the implications for economic methods, our work indicates that some caution may be required when using methods that value goods by thinking of these products as bundles of attributes. The third study is our most stark indication that consumers have a difficult time making consistent aggregations of a bundle’s attributes. When evaluating the combined bundle, consumers appear to under-value the add-ons (the pack of four replacement cartridges). This indicates an important direction of future research: how general is this apparent under-valuation? If this is in fact a general phenomenon, then methods that infer willingness to pay from valuations of attributes may be systematically biased (such as discrete choice logit models and contingent valuation models). A related direction of future work is

12 Though we have not yet tested this prediction rigorously, the idea is certainly consistent with prior findings (e.g. Chetty et al. 2009).
to investigate under what circumstances consumers aggregate attributes in a way that is more globally coherent.

Since we have found evidence against several proposed hypotheses for inattention, another direction of future work is to identify and test new hypotheses. Some candidate explanations include whether inattention (particularly in the context we study) can be explained by present bias, by limits in cognitive bandwidth from distractions when making decisions (related to ideas presented in [Benjamin et al., 2013 and Abeler and Jäger, 2015]), or by lack of visual cues (the idea being that people respond to the numbers they see, rather than the numbers they know in their head; see related work in Feldman and Ruffle, 2015). This last hypothesis may also address aggregation mistakes, as providing written aggregations removes the decision maker’s obligation to perform the calculation him or herself. By continuing this line of inquiry, we hope future work will further the understanding of how consumers think about choices that involve hidden costs and how economists use those choices to determine preferences.

References


D. J. Benjamin, S. A. Brown, and J. M. Shapiro. Who is ‘behavioral’? Cognitive ability and


